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IS PNEUMONIA INCREASING?

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It is probably impossible to answer the question: Is pneumonia increasing? The diagnosis is uncertain; an eminent authority writing to us states that whether a physician reports on a death certificate lobar pneumonia, bronchopneumonia, influenza or bronchitis must rest in many cases on his whim. But if we deal with large numbers it may be contended that the whimsy (chance?) fluctuations should average out and leave the mass statistics accurate—true if the fluctuations are really by chance and exempt from that formation and modification of mass habits which is observed in many organic fields. To throw some light on the situation a number of curves will be shown and discussed; the details of the numerical values must be reserved for publication when more space is available.

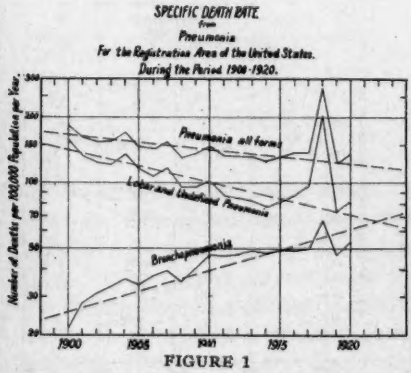

FIGURE 1

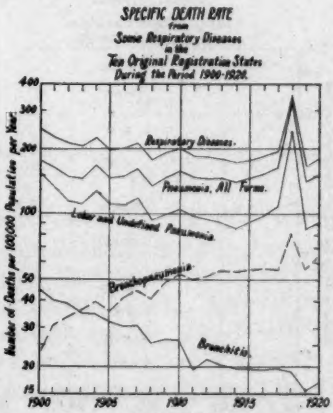

FIGURE 2

Figure 1 shows on arith-log paper the course of the mortality due to bronchopneumonia, to lobar and undefined pneumonia and to pneumonia

all forms in the U. S. registration area, 1900–1920 inclusive. Like almost all pneumonia curves for the period including 1918 these show the peaks due to influenza, moderate for bronchopneumonia but very marked for lobar and undefined pneumonia and also for pneumonia all forms. Apart from these peaks the death rate from pneumonia other than bronchopneumonia is clearly declining, sharply for lobar and undefined pneumonia, but moderately for pneumonia all forms in which bronchopneumonia with its steady increase is included.

The registration area was rapidly expanding from 1900–1920 from ten states with a population of 31 million to about three quarters of all our states and 87 millions of our population. Figure 2 shows the course of mortality when the statistics are confined to the ten original states. In these, pneumonia all forms is holding about constant (abstraction being made of the influenza year), bronchopneumonia is increasing rapidly, lobar and undefined pneumonia are declining slowly, bronchitis is dropping fast. The marked differences between the course of mortality in the registration area with its greatly widening limits and in the original ten states shows the necessity of holding the geographical limits fast if inferences are to be valid.

In figures 3 and 4 are given the age specific mortality rates from respiratory diseases and pneumonia all forms in the ten original states. Thus not only are changes of geographical area disallowed but fluctuations in the age distribution of the population are held under control. Mortality

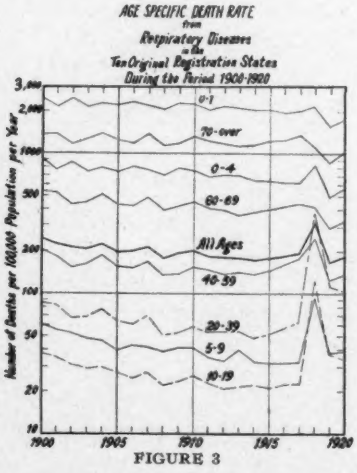

FIGURE 3

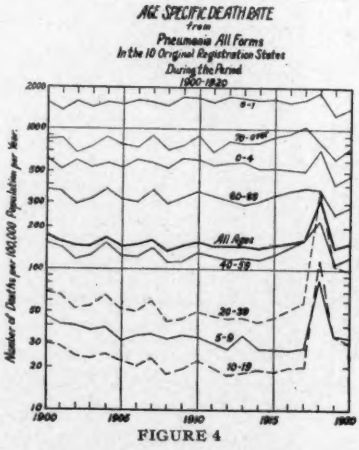

FIGURE 4

from respiratory diseases is apparently slowly falling off in all age groups except for the indications that in the groups 10–19 and 20–39 which were

hit very hard by the influenza the post-influenza rate has not come back to its pre-influenza figure. Persons over 60 had a lower respiratory and pneumonia mortality in 1918 than in the previous year. The curves for pneumonia are on the whole practically horizontal and indicate no increase or decrease in the mortality from all forms of pneumonia.

Figures 5 and 6 give a similar analysis for acute and for chronic bronchitis. For the former the mortality has been cut in two and for the

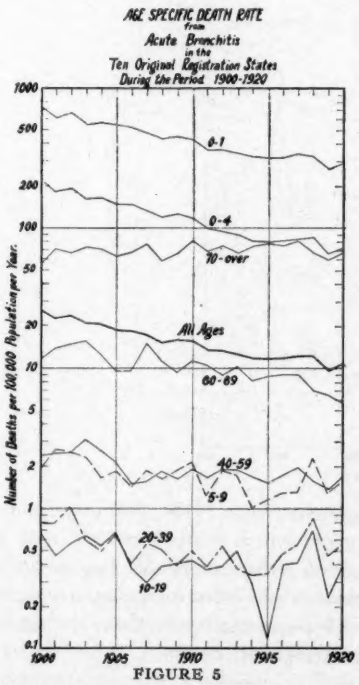

FIGURE 5

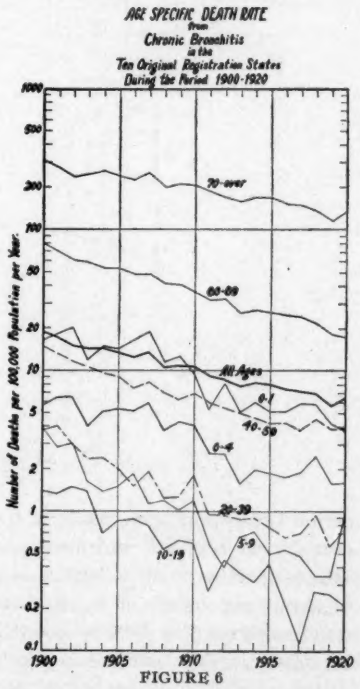

FIGURE 6

latter somewhat better than cut in two in 21 years. For chronic bronchitis the decline is at about the same rate in all age groups. For acute bronchitis the situation is somewhat mixed, the rates of decline vary and there is even a slight increase in the most advanced ages. With bronchopneumonia there is a marked increase at each age (fig. 7). Whether in reality these rates of increase or decrease are of biological or clinical importance, whether they may be due to some progressive change in the diseases, or whether they are due to shifting diagnosis, or how the rates of change should best be divided up and assessed in part to the one and in part to the other cause is very difficult to say.

In 1910 the group lobar and undefined pneumonia was subdivided and

for 11 years we may trace separately the course of mortality due to lobar and undefined pneumonia as shown in figures 8 and 9. The two groups taken together (fig. 2) exhibited a practically constant mortality in this

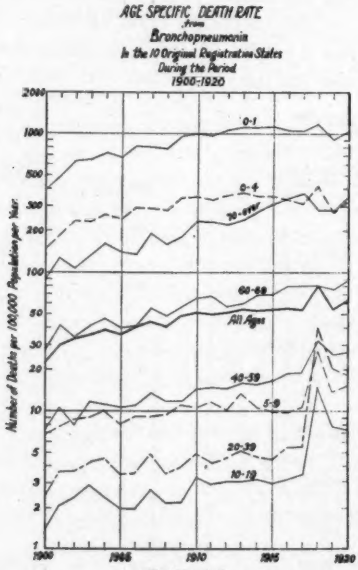

FIGURE 7

period (exception being made of the influenza year 1918); but taken individually we see that undefined pneumonia which started in 1910 with a rate at least as high as lobar has declined with remarkable precipitancy to about one-eighth of its original value while lobar pneumonia has as steadily climbed in 1920 to about double its mortality in 1910, the effect being particularly marked in the large age group 20–39.

TEN ORIGINAL REGISTRATION STATES 1900–1920

DISEASE	MEAN RATE	±S. D.	TREND	DISTURBANCY	R*	ρ
All respiratory	196.	±5.	−2.6± .4	10%	11.4	6%
Pneumonia A. F.	152.5	±2.	− .4± .3	6%	9.1	6%
Pneumonia L. and U.	105.5	±3.6	−2.1± .4	14%	9.9	9%
Bronchopneumonia	47.	±2.3	+1.7± .13	23%	3.2	7%
Bronchitis	26.7	±1.8	−1.3± .1	31%	3.4	9%

* The column headed R gives the standard deviation of the residuals after the elimination of the trend, and that headed ρ gives the coefficient of disturbancy measured after the trend is out. In all calculations the excessive rates in 1918 have been discarded or adjusted. Considering the supposed considerable effect of the weather on the incidence of and morbidity from these diseases, the reduced coefficients of disturbancy ρ are perhaps surprisingly small.

We have similar figures for each of the ten original states but shall pass them by. The numerical values for the means, standard deviations, trends, coefficients of disturbancy, etc., are also available. But here we shall offer these values only for the ten states continued.

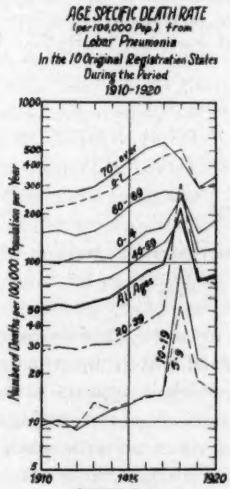

FIGURE 8

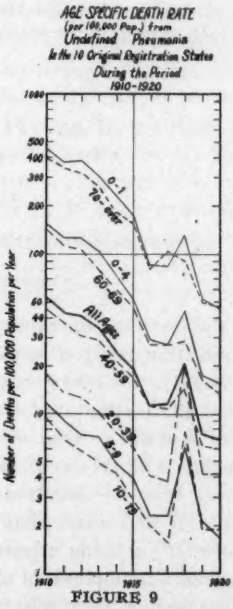

FIGURE 9

The general inference from this table which is based on a reasonably uniform population over a considerable period of time is that the mortality as reported for all respiratory diseases, for lobar and undefined pneumonia, and for bronchitis is decreasing with a statistically significant trend, whereas bronchopneumonia is increasing and pneumonia all forms is holding steady. Considering, however, the behavior of lobar and undefined pneumonia individually, the difficulties of diagnosis, the many diseases for which pneumonia is the terminal phase and others (particularly the heart group) which may complicate and terminate pneumonia, taking further into account the fact that the period 1900–1920 was one of rising prices with rising standards of living and that the economic situation may not always be so favorable, and remembering that the year 1918 (which has to be adjusted in a 21-year series but would have to be taken more fully into account in 50-year series) undoubtedly had a considerable excess of real pneumonia, all that we think it safe at present to infer is that pneumonia is neither increasing or decreasing, that the human organism

is reasonably in equilibrium with its environment with respect to this disease.

[1] This paper is a brief account of a small part of a large statistical investigation in the epidemiological situation relative to pneumonia begun by Dr. Tomanek, Fellow of the International Health Board, under Dr. M. J. Rosenau and continued in association with me. A sudden call to the Health Section of the League of Nations forced Dr. Tomanek to leave to me the composition of this work which is so largely his. E. B. W.

PRIME POWER SUBSTITUTION GROUPS WHOSE CONJUGATE CYCLES ARE COMMUTATIVE

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From the fact that the number of letters in every set of conjugate cycles of a substitution group is equal to the order of this group when each such cycle is counted for every substitution in which it appears it may be inferred that the further study of the properties of sets of conjugate cycles would be desirable.[1] One of the simplest cases presents itself when it is assumed that all the cycles in every set of conjugate cycles are commutative. Even this case involves many difficulties which have not yet been overcome. If we assume that the group G is transitive and has an order of the form p^m, p being a prime number, it is not difficult to prove that the subgroup G_1 composed of all the substitutions of G which omit a given letter must always be cyclic.

The proof of this theorem may be based upon an interesting property of the holomorph of the cyclic group of order p^m, which may be stated as follows: The largest subgroup of the holomorph of the cyclic group H of order p^m, when H is written as a regular substitution group, which satisfies the two conditions that it involves H and that its conjugate cycles are commutative is of order $p^{3m/2}$ when m is even and greater than 2. When m is odd this order is $p^{(3m-1)/2}$. If p is odd this theorem remains true when $m = 2$. In this case the subgroup of the group of isomorphisms of H which enters into the subgroup in question is completely determined by its order, while when $p = 2$ it is determined by its order together with the fact that it transforms the operators of order 4 contained in H into themselves.

By means of the theorem noted in the preceding paragraph it is not difficult to extend the result stated at the close of the first paragraph as follows: If G represents any transitive group whatever involving only commutative conjugate cycles the subgroup G_1 composed of all its substitutions which

omit a given letter must be abelian. If this were not the case G_1 would contain two non-commutative substitutions s_1 and s_2 which would transform into a power of itself every cycle of G connecting one of their common letters with a letter of G but not found in G_1. Hence such a cycle would be transformed into a power of itself by two non-commutative substitutions omitting the same letter and involving only letters of this cycle. Since this is impossible there results the following general theorem: *In every transitive substitution group involving only commutative conjugate cycles the subgroup composed of all the substitutions which omit a given letter is cyclic.*

It is not difficult to prove that the degree of G is divisible by the degree of every transitive constituent group found in a subgroup of G. In particular, the degree of G is divisible by the order of every cycle contained in G. This follows almost immediately from the fact that the group generated by any set of conjugate cycles of G is invariant under G but this group need not be contained in G. In the special case when the degree of G is of the form p^m, p being a prime number, its order is also of this form, since every prime factor of the order of a transitive group involving only commutative conjugate cycles is a divisor of the degree. The number of such non-regular groups which involve cycles of order p^m, $m > 2$ is $m/2$ when m is even and $(m-1)/2$ when m is odd. When $p > 2$ this theorem is also true when $m = 2$. That is, *the number of different groups of degree p^m which involve cycles of order p^m and have the property that each pair of conjugate cycles is composed of commutative cycles is the largest integer which does not exceed $m/2$ whenever $m > 2$.* When $p > 2$ this theorem remains true when $m = 2$.

When the largest cycle found in a group of degree p^m is of order p^{m-1} the group contains an invariant subgroup of index p having p transitive constituents of degree p^{m-1} and involving cycles of order p^{m-1}. As these transitive groups are simply isomorphic the construction of this invariant subgroup comes under the theorem noted at the end of the preceding paragraph. The cyclic subgroups of order p^{m-1} need, however, not be invariant under the entire group nor need the substitutions of the cyclic subgroups of order p^{m-1} be transformed into all their conjugates under the group by a subgroup of index p in which they appear. Hence the determination of all the possible groups in this case presents greater difficulties than in the case considered in the preceding paragraph.

[1] G. A. Miller, *Proc. Nat. Acad. Sci.*, **9**, p. 52, 1923.

AN EXTENSION OF THE THEOREM THAT NO COUNTABLE POINT SET IS PERFECT

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In 1918, Sierpinski[1] showed that if a bounded and closed point set is the sum of a countable number of mutually exclusive closed point sets then it is not connected. In the present paper I will prove the following theorem.

THEOREM 1. *If a bounded and closed point set is the sum of a countable number of mutually exclusive closed and connected point sets M_1, M_2, M_3... then not every point set of this sequence contains a limit point of the sum of the remaining ones.*

Proof. Suppose, on the contrary, that there exists a countable sequence of mutually exclusive continua M_1, M_2, M_3,...such that their sum M is closed and bounded and such that, for each n, M_n contains at least one limit point of M-M_n. By the above mentioned theorem of Sierpinski's, for each n, M_n is a maximal connected subset of M. It follows, by an easily established modification of a theorem of Zoretti's,[2] that there exists a domain D_1 containing M_1 but no point of the closed point set M_2 and such that the boundary of D_1 contains no point of M. Let G_1 denote the sequence M_1, M_2, M_3,... Since M_2 contains at least one limit point of M-M_2 it follows that there exists an *infinite* sequence G_2 whose elements are those continua of the sequence G_1 which contain at least one point without D_1 and therefore lie wholly without D_1. Let n_1 denote the smallest positive integer n, greater than 2, such that M_n lies without D_1 and therefore belongs to G_2. There exists a domain D_2 containing M_2, but no point of $D_1' + M_{n_1}$ and such that the boundary of D_2 contains no point of M. Since M_{n_1} contains at least one limit point of M-M_{n_1} there exists an infinite sequence G_3 whose elements are those continua of the sequence G_2 which lie without D_2. Let n_2 denote the smallest integer n, greater than n_1, such that M_n lies without D_2 and therefore belongs to G_3. There exists a domain D_3 containing M_{n_1}, but no point of $D_1' + D_2' + M_{n_2}$, and such that the boundary of D_3 contains no point of M. There exists an infinite sequence G_4 whose elements are those continua of the sequence G_3 which lie without D_3. Let n_3 denote the smallest integer n, greater than n_2, such that M_n lies without D_3. There exists a domain D_4 containing M_{n_3} but no point of $D_1' + D_2' + D_3' + M_{n_3}$ and such that the boundary of D_4 contains no point of M. This process may be continued. Thus there exists a countably *infinite* set of mutually exclusive domains D_1, D_2, D_3,...such that (a) every point set of the sequence G_1 is in some

domain of the set K and (b) each domain of the set K contains some point set of the sequence G_1. It follows that M is not covered by any proper subset of the set of domains K. But, since M is closed and bounded, this contradicts the Heine-Borel-Lebesgue Theorem. The truth of Theorem 1 is therefore established.

That the above theorem does not remain true if the stipulation that M be bounded is omitted, may be seen with the aid of the following example.

Example 1. For each positive integer n, let K_n denote the set of all decimal fractions in which there are just n digits equal to 1 and in which all the other digits are equal to 0. Let $d_{1n}, d_{2n}, d_{3n}, \ldots$ denote the fractions of the set K_n. For each n and m, let A_{mn} denote the point $(1/n, d_{mn}/n)$. Let K denote the point set consisting of the point (1, 0) and all points A_{mn} for all values of the positive integers m and n. The points of the countable set K may be designated $P_1, P_2, P_3, \ldots$ For each n let OP_n denote the straight line interval whose end-points are O and P_n and let M_n denote the ray into which the point set $(OP_n - O)$ is thrown by an inversion of the plane about a circle of radius 2 with center at O. Let M denote the sum of all the point sets of the sequence $M_1, M_2, M_3, \ldots$ The point set M is closed and, for each n, M_n contains at least one limit point of $(M\text{-}M_n)$.

That Theorem 1 becomes false if the stipulation that M be closed is omitted may be seen with the help of an example given by Miss Anna M. Mullikin in her Doctor's dissertation.[3] That it becomes false if the stipulation that each point set of the sequence $M_1, M_2, M_3, \ldots$ be connected is omitted may be seen with the aid of the following example

Example 2. For each positive integer n, let P_n^n denote the point on OX whose abscissa is $1 - 1/n$. For each pair of positive integers m and n, such that $n > 1$ and $m > n$, let P_m^n denote the point on OX whose abscissa is $[1 - 1/(n-1)] + [1/(n-1) - 1/n]/m$. Let M_1 denote the point set composed of the points P_1 and $(1, O)$. For each n greater than 1, let M_n denote the point set composed of the $n - 1$ points $P_n^2, P_n^3, \ldots P_n^n$.

If $M_1, M_2, M_3, \ldots$ is a sequence of point sets let B denote the statement that their sum is not connected and let A denote the statement that not every point set of this sequence contains at least one limit point of the sum of the remaining ones. Clearly A implies B, but B does not imply A. In other words Statement B is weaker than Statement A. Sometime last year I found that if the word "bounded" is omitted from the statement of Theorem 1, the theorem remains true provided the conclusion (Statement A) is replaced by the weaker statement B. This result was announced at the Summer meeting of the American Mathematical Society, September 6, 1923. The same result has been established by S. Mazurkiewicz in an article in Volume V of *Fundamenta Mathematicae*. This volume bears the date 1924, but a reprint of the article left the press before

the appearance of the entire volume, just how long before, I do not know.[4]

[1] Sierpinski, W., "Un theoreme sur les ensembles continus," *Tohoku Math. J.*, **13**, 1918 (300).

[2] Zoretti, L., "Sur les fonctions analytiques uniformes," *J. Math. pures appl.*, **1**, 1905 (9–11).

[3] Certain theorems relating to plane connected point sets, *Trans. Amer. Math. Soc.*, **24**, 1922 (145), Fig. 1.

[4] I submitted my proof for publication in *Fundamenta Mathematicae*, the manuscript being mailed Sept. 28. Sometime in November I received the reprint of the article by Professor Mazurkiewicz.

CONCERNING THE PRIME PARTS OF CERTAIN CONTINUA WHICH SEPARATE THE PLANE

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Hans Hahn[1] has recently introduced the notion of *prime parts* of a continuum. If P is a point of a continuum M, the prime part of M which contains P is the set of all points $[X]$ belonging to M such that for every positive number e there exists a finite set of irregular[2] points, $P_1, P_2, P_3 \ldots P_n$, of M such that the distances $PP_1, P_1P_2, P_2P_3, \ldots P_nX$ are all less than e. Among other things, Hahn shows that if M is *irreducibly* continuous between the points A and B and it has more than one prime part then its prime parts may be ordered in a certain manner and there exists, between them and the points of a straight line interval, a one to one correspondence which preserves order and which is, in a certain sense, continuous.

In the present paper the following theorem will be established.

THEOREM 1. *If, in a plane S, M is a bounded continuum which has more than one prime part and no prime part of M separates S, then, in order that $S-M$ shall be the sum of two mutually exclusive domains such that each prime part of M contains at least one limit point of each of these domains, it is necessary and sufficient that the set whose elements are the prime parts of M shall be a simple closed curve of prime parts in the sense that it is disconnected by the omission of any two of its elements which are not identical.*

In my proof of this theorem I will make use of a number of lemmas.

LEMMA 1. *The outer[3] boundary of a bounded domain is a continuum which is not disconnected by the omission of any one of its points.*

That the boundary of a complementary domain of a bounded continuum is itself a continuum has been proved by Brouwer.[4] The truth of Lemma 1 is established in a paper which I have recently submitted for publication in *Fundamenta Mathematicae*.[5]

LEMMA 2. *If H is a continuum which contains a limit point of the bounded domain D and a limit point of the unbounded domain which is complementary to the boundary of D then H contains a point of the outer boundary of D.*

The truth of Lemma 2 may be easily established.

Definition. If D is a bounded complementary domain of a bounded continuum M then by the outer boundary of D *with respect to the prime parts of* M is meant the set K composed of all prime parts $[p]$ of M such that p contains at least one limit point of D and at least one limit point of the unbounded domain which is complementary to the boundary of D.

In view of Lemma 2 it is clear that the set of prime parts K so defined consists of all those prime parts $[p]$ of M such that p contains at least one point of the point set which forms the outer boundary of D.

Definitions. If p is a prime part of M and G is a set of prime parts of M then p is said to be a *limit* prime part of G, or a *limit* element of G, if for every positive number e there exists at least one point which is at a distance less than e from some point of p and which belongs to some prime part of M which belongs to the set G and is distinct from p. A set of prime parts (of M) G is said to be *closed* if every limit element of G belongs to G. It is said to be *connected* if it is not the sum of two mutually exclusive subsets neither of which contains a limit element of the other one. If a set of prime parts of M is both closed and connected it is said to be a *continuum* of prime parts of M.

If p and q are two distinct prime parts of a continuum M, then by a *simple continuous arc* of prime parts of M with p and q as *extremities* is meant a set G of prime parts of M such that (*a*) G is disconnected by the omission of any one of its elements which is distinct from p and from q, (*b*) the point set obtained by adding together the points of all the prime parts of M which belong to G is a bounded point set.

LEMMA 3. *If D is a bounded complementary domain of a bounded continuum M and K is the outer boundary of D with respect to the prime parts of M then K is a continuum of prime parts of M and if p is a prime part of M belonging to the set K then K-p is a connected set of prime parts of M.*

Proof. Let B denote the point set which forms the outer boundary of D. Since each point of B belongs to some prime part of M which belongs to the set K and each prime part of M belonging to the set K contains a point of B and B is a closed and connected set of points, it follows[6] that K is a closed and connected set of prime parts of M. I will proceed to show that if p is a prime part of M belonging to the set K then K-p is connected. Suppose, on the contrary, that K-p is the sum of two mutually separated[7] sets of prime parts (of M) H and N. Then clearly $H + p$ and $N + p$ are both closed and connected sets of prime parts and they have in common only the prime part p. Let X denote a point of D and let Y denote a point of E, the unbounded domain which is complementary

to B. Let N^* denote the set of points obtained by adding together the points of all the prime parts of M belonging to the set $N + p$ and let H^* denote the point set obtained in the same way from $H + p$. Since the continua N^* and H^* have in common only the continuum p and $N^* + H^*$ separates X from Y therefore[8] either N^* or H^* separates X from Y. Suppose that H^* does. Let D_X and D_Y denote the complementary domains of H^* that contain X and Y, respectively. Clearly D_X contains D. Let q denote a prime part of M that belongs to the set N. The prime part q contains a limit point of D and therefore of D_X. Since the continuum q contains a limit point of D_X, but no point of the boundary of D_X, therefore q is a subset of D_X. But some point of q is a limit point of E. Thus D_X contains a point of E and therefore, since E is connected and contains no point of the boundary of D_X, E is a subset of D_X. Thus Y belongs to D_X, contrary to supposition. Similarly the supposition that N^* separates X from Y would lead to a contradiction. The truth of Lemma 3 is therefore established.

LEMMA 4. *If the connected point set K is a proper subset of the bounded continuum M there exists at least one point of M-K whose omission does not disconnect M.*

Proof. Let P denote some definite point of M-K. Suppose M-P is not connected. Then it is the sum of two mutually separated point sets H and N where H contains the connected point set K. The point sets $H + P$ and $N + P$ are continua with only P in common. The continuum $N + P$ has[9] at least two non cut points. Thus it contains a point X distinct from P such that $N + P - X$ is connected. Since the connected sets $N + P - X$ and $H + P$ have the point P in common their sum is connected. The truth of Lemma 4 is therefore established.

LEMMA 5. *If K is a closed and connected set of prime parts of the bounded continuum M and H is a connected proper subset of K then the set K-H contains a prime part of M whose omission does not disconnect M.*

LEMMA 5 may be easily proved with the aid of Lemma 4 and certain results established in P. C.

In P. C. I showed that if, for a given bounded continuum M, the word "point," as used in my paper *On the foundations of plane analysis situs*,[10] is interpreted to mean "prime part of M" and the word "region," as used therein, is interpreted to mean a certain sort of collection of such "points" then, for the space S^* consisting of all "points," Axioms 1 and 4 and Theorem 4 of that paper hold true and a prime part p will be a limit prime part of a set of prime parts K if, and only if, every region which contains p contains at least one element of the set K which is distinct from p. For an indication of material sufficient for a proof, based on this result, of the following Lemmas 6 and 7 see Page 139 of F. A. and Page 342 of my paper Concerning simple continuous curves.[11]

LEMMA 6. *If M is a bounded continuum and K is a simple continuous arc of prime parts of M then every closed and connected subset of K which contains more than one prime part of M is also a simple continuous arc of prime parts of M.*

LEMMA 7. *If M is a bounded continuum, pq is a simple continuous arc of prime parts of M with p and q as its extremities, x is a prime part of M belonging to pq and H is a set of prime parts of M belonging to pq then p is a limit element of the set H if and only if every simple continuous arc of prime parts of M which contains x and is a subset of pq (but does not have x as an extremity) contains at least one element of the set H which is distinct from x.*

LEMMA 8. *If M is a bounded continuum then no simple continuous arc of prime parts of M separates the plane unless the plane is separated by some prime part of M which belongs to that arc.*

Proof. Suppose, on the contrary, that M is a bounded continuum, p and q are prime parts of M, pq is a simple continuous arc of prime parts of M from p to q and that, though no prime part of M belonging to the arc pq separates the plane S, nevertheless pq separates S. Let H denote the point set obtained by adding together the points of all the prime parts of M which belong to the arc pq. Let D denote a bounded complementary domain of H and let B denote its outer boundary with respect to the prime parts of M. Since S is not separated by any prime part of M belonging to the set pq therefore B is a closed and connected set of prime parts of M containing more than one prime part of M. Hence, by Lemma 6, B is itself a simple continuous arc of prime parts of M. It follows that B contains uncountably many prime parts of M such that the omission of any one of them disconnects B. But this is contrary to Lemma 3.

Proof of Theorem 1. I will first show that the condition of Theorem 1 is necessary. By hypothesis, M has just two complementary domains D_1 and D_2 and every prime part of M contains at least one limit point of D_1 and at least one limit point of D_2. Let p and q denote two distinct prime parts of M. By a theorem established in P. C., p and q are the extremities of a simple continuous arc of prime parts of M. Let pq denote such an arc and let r denote a prime part of M which belongs to pq and is distinct from p and from q. The point set $M-r$ is connected. For suppose, on the contrary, that it is the sum of two mutually separated sets H_1 and H_2. Then it can easily be seen that $H_1 + r$ and $H_2 + r$ are two bounded continua and that the set of points common to them is the continuum r. Furthermore neither $H_1 + r$ nor $H_2 + r$ separates S. Indeed S is not separated by any closed and connected point set which is obtained by adding together the points of all the prime parts of some continuum of prime parts of M. For let N denote such a point set. Let X denote a point of D_1 and let D denote that complementary domain of N which contains X. Since D_1 is connected and N is a subset of M, D contains the whole of D_1

and therefore at least one point (and hence the whole) of every prime part of M which is not a subset of N. But every such prime part of M contains a limit point of D_2. Therefore D contains at least one point of D_2 and therefore all of D_2. Thus D is identical with S-N. Hence N does not separate S. In particular, neither $H_1 + r$ nor $H_2 + r$ separates S. It follows, by a theorem of Janiszewski's cited above, that S is not separated by the sum of the continua $H_1 + r$ and $H_2 + r$. But this is contrary to hypothesis.

Since M-r is a connected set of points it follows that if S^* denotes the set of all prime parts of M then S^*-r is a connected set of prime parts. Thus S^*-r is a domain[12] with respect to S^*. Therefore[6] p and q are the extremities of a simple continuous arc (of prime parts of M) c which lies wholly in S^*-r and therefore does not contain r. It is easy to see that there exists an interval d of the arc pq and an interval e of the arc c such that d and e have in common only their extremities. It follows, by a theorem of Janiszewski's,[8] that $d + e$ separates S. Hence $d + e$ is identical with M and therefore p and q are the extremities of the arcs d and e. Hence $S^* - (p + q) = [d - (p + q)] + [e - (p + q)]$ and therefore $S^* - (p + q)$ is not a connected set of prime parts. It follows that $M - (p + q)$ is not a connected set of points.

That the condition of Theorem 1 is also sufficient may be shown as follows. By hypothesis M is a bounded continuum which has more than one prime part and which is disconnected by the omission of any two of its prime parts which are not identical. Let S^* denote the set of all prime parts of M. Since Axioms 1 and 4 and Theorem 4 of F. A. hold true for S^* in the sense indicated above in the discussion preceding the statement of Lemmas 6 and 7, it follows, by Theorem 4 of my paper Concerning Simple Continuous Curves,[11] that if p and q are any two prime parts of M then S^* is the sum of two simple continuous arcs (of prime parts of M) which have p and q as their extremities but which have in common no elements except p and q. Let t_1 and t_2 denote these arcs. Let t_1^* denote the set of points obtained by adding together all the points of the prime parts of the set t_1 and let t_2^* denote the point set obtained in the same way from t_2. By Lemma 8, neither of these point sets separates the plane. But they have in common only the two continua p and q and these two continua have no point in common. It follows, by a theorem of Miss Mullikin's,[13] that S-M is the sum of two mutually exclusive domains, D_1 and D_2. Every prime part of M contains at least one limit point of D_1 and at least one limit point of D_2. For suppose, on the contrary, that there exists a prime part (of M) p which does not contain a limit point of, for instance, D_1. Let K denote the set of all those prime parts of M each of which contains at least one limit point of D_1. From a theorem[14] established in P. C., it follows that K is closed. Hence, by Lemma 7,

there exists an arc t (of prime parts of M) which contains p, but does not have p as one of its extremities, and which contains no point of K. Let s denote the other arc of prime parts of M which has the same extremities as t. Let H denote the point set obtained by adding together the points of all those prime parts of M which belong to t but are distinct from its extremities. Clearly the two point sets $D_2 + H$ and D_1 are mutually separated. Hence s separates space. But this is contrary to Lemma 8.

[1] "Über irreduzible Kontinua," *Wien. Ber.*, **130**, 1921 (217–250).

[2] An *irregular* point of M is a point at which M is not "connected im kleinen." Cf. Hahn, H., *Wien. Ber.*, **123**, 1914 (2433).

[3] By the *outer* boundary of a bounded domain D is meant the boundary of that complementary domain of D which is unbounded.

[4] *Math. Ann.*, **69**.

[5] The title of this paper is "Concerning the Sum of a Countable Infinity of Continua in the Plane."

[6] See my paper "On the Prime Parts of a Continuum." This paper has been recently submitted for publication in *Mathematische Zeitschrift*. It will be referred to as P. C.

[7] Two sets are said to be mutually separated if they have no element in common and neither of them contains a limit element of the other one.

[8] Cf. Janiszewski, S., *Sur les coupures du plan faites par les continus, Prace matem.-fizyczne*, **26**, 1913.

[9] See my paper "Concerning the Cut-points of Continuous Curves and of Other Closed and Connected Point Sets," These PROCEEDINGS, **9**, 1923 (101–106). A cut-point of a connected point set M is a point P whose omission disconnects P.

[10] *Trans. Amer. Math. Soc.*, **17**, 1916 (131–164). This paper will be referred to as F. A.

[11] *Trans. Amer. Math. Soc.*, **21**, 1920 (333–347).

[12] If S^* is the set of all prime parts of a continuum M and K is a closed subset of S^* then the set of prime parts S^*-K is said to be a domain with respect to S^* provided it is non-vacuous and connected.

[13] Anna M. Mullikin, "Certain Theorems Relating to Plane Connected Point Sets," *Trans. Amer. Math. Soc.*, **24**, 1922 (160).

[14] This theorem is to the effect that if p is a prime part of a bounded continuum M and $p_1, p_2, p_3, \ldots$ is a sequence of prime parts of M and for every positive number e there exists a positive number d_e such that, for every n greater than d_e, p_n contains at least one point which is at a distance less than e from some point of p, then for every positive e there exists a positive number d_e^* such that, for every integer n greater than d_e^*, *every* point of p_n is at a distance less than e from some point of p.

CONCERNING THE DIVISION OF THE PLANE BY CONTINUA

By John Robert Kline

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Communicated April 1, 1924

In a paper recently published in the *Fundamenta Mathematicae*,[1] I proved that if M is a closed connected set which contains more than one point and remains connected (in the weak sense),[2] upon the removal of any connected subset g, then M is a simple closed curve. In the same number of the Fundamenta, Kuratowski[3] proved that if C is a bounded continu, which remains connected in the strong sense upon the removal of any subcontinu, then C is a simple closed curve. Kuratowski's theorem would not be true provided it were merely required that C remain connected in the weak sense upon the removal of any subcontinu. For consider the set $M = \sum_{i=1}^{4} M_i$, where M_1 is the point set composed of $y = \sin 1/x$ for $o < x \leq 1/\pi$, M_2 the interval $(1/\pi, o)$ to $(1/\pi, -2)$, M_3 is the interval $(1/\pi, -2)$ to $(0, -2)$ while M_4 is the interval $(0, -2)$ to $(0, +1)$. The set M does not remain connected in the strong sense provided the interval from $(0, 0)$ to $(0, -2)$ is removed. But the set M remains connected in the weak sense no matter what subcontinu is removed. In the present paper I shall consider bounded decomposable[4] continua lying in a plane S, and having the property that they remain connected in the weak sense upon the removal of any sub-continu. I shall prove that any continu satisfying these conditions divides its plane and that unless it is a simple closed curve, it cannot be a continuous curve.[5]

THEOREM A. *Suppose M is a bounded decomposable continu lying in a plane S such that if g is any subcontinu of M, then $M-g$ is connected.*[6] *Under these hypotheses, M disconnects its plane S.*

Proof: Suppose M does not disconnect its plane. Now as M is decomposable, then $M = M_1 + M_2$, two continua, each different from M. Hence neither of the sets, M_1 and M_2, is a proper subset of the other one. Neither M_1 nor M_2 can divide the plane S. For suppose $S - M_1 = G_1 + G_2$, two mutually exclusive sets neither of which contains a limit point of the other one. Now, by supposition, $S - (M_1 + M_2)$, a subset of $S - M_1$, is connected. Hence $S - (M_1 + M_2)$ must be a subset either of G_1 or of G_2. Let us suppose it is a subset of G_1. Then G_2 is a subset of M_2. Let P be any point of G_2. As no point of G_2 is a limit point of G_1, there is a circle C enclosing P such that neither C nor its interior contains a point of G_1. Hence neither C nor its interior contains a point of $S - (M_1 + M_2)$. Hence C plus its interior is a subset of M. Let J be any simple closed curve lying entirely within C and hence belonging to M. As there are points of M within J and also points of M without J, then J is a continu whose removal fails to leave M connected in the weak sense. Hence the supposition that M_1 disconnects the plane has lead to a contradiction.

In like manner M_2 fails to disconnect S. Several cases may arise:

Case I. D, the common part of M_1 and M_2 is connected. Then as M_1 and M_2 are closed, D is a continu. Then $M - D = (M_1 - D) + (M_2 - D)$. Neither of these sets is vacuous nor can either contain a limit point of the other one. Hence D divides M, contrary to hypothesis.

Case II. D, the common part of M_1 and M_2 is not connected. Then as neither M_1 nor M_2 divides S, it follows from a theorem due to Miss Anna Mullikin,[7] that $M_1 + M_2$ must divide S into at least two parts.

THEOREM B. *Suppose that M is a bounded decomposable plane continu such that if g is any subcontinu of M, then M-g is connected in the weak sense. Under these hypotheses, if M is a continuous curve, then M is a simple closed curve.*

Proof. Suppose M is a bounded continu satisfying the hypotheses of our theorem. Let g be any subcontinu of M. Then M-g satisfied R. L. Moore's definition of a maximal connected subset of the open subset M-g.[8] Hence by a theorem due to Moore, M-g is arcwise connected. As an arc is a continu, then any two points of M-g lie together in a closed and connected subset of M-g. Hence M-g is connected in the strong sense. Hence by the theorem due to Kuratowski, M is a simple closed curve.

[1] Cf. my paper "Closed Connected Sets which Remain Connected upon the Removal of Certain Connected Subjects." *Fund. Math.*, **5**, pp. 3–10, 1924.

[2] A point set M is said to be *connected* (*in the weak sense*) if however, it be divided into two mutually exclusive sets, one of them contains a limit point of the other one. A *continu* is a set that is both closed and connected. A set M is said to be *connected in the strong sense* (*semi continu* in terminology of Kuaster and Kuratowski) if every pair of points A and B belonging to M, lie together in a closed connected subset of M.

[3] Cf. C. Kuratowski, "Contribution a L'etude de continus de Jordan," *Fund. Math.*, **5**, p. 119, 1924.

[4] A continu which is not the sum of two continua each different from itself is said to be *indecomposable*. Every indecomposable continu M is such that if g is a subcontinu of M, then M-g is connected. It is evident that it is possible to construct an indecomposable plane continu which is bounded and fails to divide its plane.

[5] A *continuous curve* is a closed connected set M which is connected im kleinen at every one of its points.

[6] Here after in this paper, unless otherwise stated, "connected" and "connected in the weak sense" will be considered as synomymous terms.

[7] Cf. Anna M. Mullikin, "Certain Theorems Relating to Plane Connected Sets," *Trans. Amer. Math. Soc.*, **24**, p. 154, 1922. Miss Mullikin's result is as follows: *If M_1 and M_2 are two closed connected bounded point sets neither of which disconnects a plane S, then a necessary and sufficient condition that their sum M shall disconnect S is that M, the set of points common to M_1 and M_2 be not connected.*

[8] Cf. R. L. Moore, "Report on Continuous Curves from the Viewpoint of Analysis Situs," *Bull. Amer. Math. Soc.*, **29**, p. 294, 1923. According to Moore, a point set M is said to be *arcwise connected* if every two distinct points of M are the extremities of a simple continuous arc which lies wholly in M. The point set K is said to be an *open subset* of M if K is a subset of M and M-K is either vacuous or closed. A *maximal connected subset* of a point set K is a connected subset of K which is not a proper subset of any other connected subset of K. Moore's result is as follows: *In order that a continuum M should be a continuous curve it is necessary that every maximal connected subset of an open subset of M should be arcwise connected.*

THE PARTIAL GENETIC INDEPENDENCE IN SIZE OF THE VARIOUS PARTS OF THE BODY

By F. B. Sumner

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Communicated February 26, 1924

Castle's article in the January number of these PROCEEDINGS is to be welcomed as defining somewhat more clearly his position with regard to size inheritance. I feel tempted to say his *present* position, since I find it hard to reconcile his more recent statements with some of those contained in his earlier article dealing with this subject.[1] Does he, for example, still hold unreservedly that "the view of the genetic independence in size of the various parts of the body is a sporadic relapse into preformationism, such as was perhaps excusable to the Grecian mind" (p. 19)?

The reader is, of course, not interested in questions of forensic advantage, nor in changes of personal viewpoint. I think, however, that there are important biological problems involved in the present discussion—sufficiently important to warrant a further effort on my part to clear up certain points at issue.

Castle now explains that in making the claim that "the genetic factors which affect size in mammals are *general* in their action, *exclusively* so," he had reference to factors affecting *total* size, and not "*any detail of size in any part of the body*." Despite the illustrative cases which he cites, I am unable to comprehend this distinction. All parts of the body contribute to total size, some doubtless more than others. Is not the difference merely one of relative magnitude?

Castle puts aside as irrevelant the "slight quantitative variations in ear-length, foot-length and tail-length" which I have studied in *Peromyscus*. Yet he has no hesitation in including ear-length and the length of the leg bones in rabbits. The differences in *Peromyscus* are not so slight as he supposes—at least when *races* are compared. Thus the tail of *Peromyscus maniculatus hylaeus*, from Alaska, has a mean length which is more than 10% greater than that of the remainder of the body, while the tail of *P. m. rufinus*, from Arizona, has a mean length which is only 75% of the body length. These differences are quite comparable in magnitude with those distinguishing the Polish and Flemish Giant rabbits. It can hardly be seriously maintained that such differences do not materially affect the total size of the body. They very greatly affect the "total length," as commonly measured by mammalogists, and must appreciably affect the weight. Within any single race of *Peromyscus*, to be sure, the range of variation is much less than this, but the same, we must remember, is true of Castle's rabbits.

In my previous article in these PROCEEDINGS (Nov., 1923), I attributed the high correlations which Castle obtained, between various measurable parts of the body, to the mixing of widely different races, together with hybrids between these. I pointed out that, in such a case, one would necessarily "obtain a high correlation between any two measured parts, provided that these varied in the same direction in the large and small races which were mixed, and that the mean differences between these races were great in proportion to the independent variations occurring within each single race."

Castle replies that in one case (femur-humerus) he has "treated each group separately and found that it makes no great difference." This statement of the case is somewhat misleading since there is but a single table for all animals of "pure race," taken collectively (table 25). Thus combined, they naturally show a very high correlation (0.980) between femur and humerus. This is not the whole story, however. If we treat these races separately, as we are able to do by referring to Castle's table 33, we obtain quite different results. The 18 Polish rabbits give a correlation of 0.813, between femur and humerus, while in the 8 Himalayan the correlation falls to 0.520. (It was hardly worth while to compute a coefficient for the 4 Flemish Giants.)

But a study of certain other correlations proves to be still more instructive. Castle has wisely published his original data, so that the value of his work is unaffected by the truth or falsity of any particular interpretations. The individual measurements of all of his animals are available for any computations which the reader desires to undertake. Accordingly, I have thought it worth while to compute coefficients for a few of the pairs of characters, in certain groups, taken separately.[2] The following table gives the resulting coefficients, along with Castle's figures, which were based upon all of the groups combined:

	POLISH (17)	POLISH & FLEMISH F_1 HYBRIDS (27)	ALL GROUPS COMBINED (CASTLE)
Skull-femur	.565	.503	.871
Femur-ear	.244	.203	.828
Tibia-weight	.509	.037	.758

These six group coefficients average only 0.343, while the mean of Castle's figures is considerably more than twice as much (0.819). I think it is plain that such a segregation of the diverse elements of this population does make *a great deal of difference* in the results. I think, too, that the figures fully sustain my contention that Castle's exceptionally high correlation coefficients were due to the mixing into a single statistical "population" of races which differed from one another in appropriate ways. Quite different results—even negative correlations—could be obtained,

as I pointed out in my previous article (these PROCEEDINGS, Nov., 1923), by selecting two other races, showing size relations of an opposite sort. Why are these negative correlations any less significant biologically than the positive ones to which Castle attributes so much theoretic importance? The mixing is no more "mechanical" in one case than in the other. To combine, in a single table, Polish and Flemish Giant rabbits can hardly be called "genetic mixing," despite the fact that these animals happened to have been mated together during life.

Castle[3] refers to the ductless glands, as providing a probable mechanism for the coördination of growth in animals. This seems to be a very reasonable view, for which there is already considerable evidence. It is worth pointing out, however, that the control of growth by hormones, or other materials contained in the general blood stream, is in no way inconsistent with the production of highly specific local effects. The influence of the sex hormones upon the secondary sexual characters of mammals and birds has long been familiar to us. A more recent instance has been furnished by Hammett in his studies of thyroid and parathyroid removal in rats.[4]

Hammett's general conclusion is that "thyroid lack causes disproportionate differential development." While the growth of all organs in his experimental series was retarded,[5] the effect was widely different for the 23 different parts measured, the percentages of increase varying from −44.5 (an actual decrease), for the thymus of the females, to +48.0, for the epididymis of the males. It is of interest that the growth of the brain was affected to a far greater extent than the growth of the spinal cord.

Surely, the initial local differences which determine such widely different reactions to the same chemical or physical stimulus are themselves to be included in the category of "genetic agencies affecting size." While we must grant Castle's contention, so far as to admit that the agencies which affect size are *largely* general in their action, we must, I think, also admit that they are, to a considerable degree, local and specific. As to the question which of these tendencies preponderates, it would, I think, be rash to generalize in the present state of our knowledge.

[1] *Carnegie Inst. Publ.*, No. **320**, 1922.

[2] I purposely avoided pairs of characters which were closely related anatomically. Bones such as the femur and tibia, or even the femur and humerus, would be expected to show a high degree of correlation, even in series of animals which were fairly homogeneous in size. Otherwise, the selection was made without any foreknowledge of the probable results.

[3] 1922, p. 19.

[4] *J. Hered.*, Oct., 1923; *Amer. J. Physiol.*, Dec., 1923.

[5] With the exception of the hypophysis in the males.

ARE THE VARIOUS PARTS OF THE BODY GENETICALLY INDEPENDENT IN SIZE?

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Communicated March 4, 1924

Dr. Sumner has kindly sent me his manuscript with an invitation to comment on it, which I gladly accept. I am not greatly concerned over seeming to have changed my views. If the views which I had previously expressed were erroneous, I should be as anxious as anyone to have them corrected. As to the specific sentence which I am invited to re-indorse or retract, I will restate it. *It would be as fanciful and foolish for us to maintain the complete genetic independence of the various parts of the body as it was for an ancient Greek philosopher to hold that those parts were wholly independent in their origin.*

I am still unconvinced that slight differences in ear-length, foot-length and tail-length in Peromyscus make any material difference in the total weight of the carcass. But Sumner can easily settle the matter by detaching the organs in question and weighing them and thus estimating the differences in weight of these organs in different races. Shepherds apparently do not consider the sheep's tail an important element in total body weight, or they would not dock their lambs as they do habitually, even those which are sold by weight. The treatment of ear-length which I have made in rabbits differs fundamentally from that recommended by Sumner. I have used ear-length as an *index* of total size, not as a factor in producing it. This treatment is justified by the high correlation shown to exist between ear-length and weight.

The subject of correlation brings us to the really important part of Sumner's communication. I had not fully appreciated his position concerning it before. In my original paper, I put into a single correlation table the measurements for each of the three pure races studied and for their F_1 and F_2 hybrids. Sumner objects to this procedure. He insists that each race should be studied as a group by itself and he shows that when this is done, smaller coefficients are obtained. I was fully aware that such would be the case, and it did not occur to me that in his previous communication Sumner was alluding to a fact so elementary as this. I pointed out in my original paper that other things being equal the greater the *range* of variation, the greater would be the correlation indicated, because accidents of development would have less weight.

Sumner's procedure in dealing with my data is to deal with each racial group by itself and then *average* the coefficients thus obtained, in order to get a general measure of correlation within the population. This sounds

reasonable, but is it? What do we desire to learn from our correlation table? What I sought to learn was whether, *with change in general body size through genetic (not environmental) agencies, there was or was not a corresponding change in the different parts of the body.* In order to get a clear answer to this question it was desirable that the differences in general body size within the population studied be as great as possible, so I started with races as pure as could be obtained, and having a maximum difference in body size. I then produced a population by crossing these widely differing races, both F_1 and F_2 generations being obtained. Combining all groups, I found a high degree of correlation between body weight, ear-length, and various bone dimensions. When F_1 or F_2 groups were studied separately the correlation was only slightly less than for the entire population, owing to the lesser total range. When the pure races were studied as a single group apart from the hybrids, the highest correlation of all was found. Now Sumner would apply a method which would make this correlation appear *least* of all, by studying each pure race by itself and then averaging these results. His method is not legitimate for this reason. We are looking for genetic correlations. *There is no genetic correlation within a pure line,* as Johannsen has shown, for there is no genetic variation, only phenotypic variation. Within a race homozygous for all genetic factors influencing size, the only variations in size which will be observable will be due to accidents of development, or in other words the local environment. Averaging any number of zero correlations would of course give only a zero result. Now my "pure races" of rabbits were not "pure lines" but they come as close to it probably as any races of rabbits in existence, for they have been long selected by breeders in opposite directions for extreme conditions of size.

If races of rabbits which represent extremes of general size differ only in respect to genetic agencies and further *identical* genetic agencies, then we should expect to find correlation *greatest* when the races are tabulated in a single correlation table, as is the case. And we should expect to find no great falling off in the correlation in an F_2 population produced by crossing the pure races, as also is the case. If the F_2 group *did* show a considerable falling off in correlation, greater than might be explained as due to the shortened range, then we might assume that *different* genetic agencies made one race small and the other big, but this we are not justified in doing, for the correlation changes little.

I claim therefore to have demonstrated that in rabbits the *only* genetic agencies of any consequence which affect size are those which affect size generally.

ON THE LONGEVITY OF SPATHIDIUM SPATHULA WITHOUT ENDOMIXIS OR CONJUGATION

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Communicated March 27, 1924

Studies on representatives of the various groups of Protozoa have served to emphasize the nicety of the adaptation of species and races to particular environmental conditions. Even such relatively ubiquitous forms as *Paramecium aurelia* and *Paramecium caudatum*, which are able to survive temporarily under very diverse conditions, gradually succumb to prolonged subjection to media which at first seem entirely suitable. Ample evidence of this is afforded by the history, during more than four score years, of the study of Paramecia cultures, since increased refinements of culture methods have extended the apparent potentialities of life from a few days and generations to more than fifteen years and nearly as many thousand generations in *Paramecium aurelia*—in fact, we may say, indefinitely. And this has relegated the so-called "cycle" of this organism to the realm of fancy.[1]

It would seem that a quite similar history has been briefly recapitulated in this laboratory in the studies on *Spathidium spathula* during the past four years. The majority of the cultures carried by Woodruff and Spencer showed what heretofore would have been interpreted as "cycles" which culminated in death unless conjugation was permitted, though certain cultures continued to live for over 500 generations without conjugation or endomixis. The drift toward longer life was so evident as more and more experience was gained in culturing the animals, that Woodruff and Spencer and Moore interpreted the data as clearly indicating the ability of Spathidium to live indefinitely without conjugation or endomixis under entirely suitable cultural conditions.[2] This conclusion is so far-reaching in its import for the interpretation of the life histories of the Protozoa in general, and the Infusoria in particular,[3] that we have investigated it further in Spathidium. The present paper gives a brief statement of the results.

The animals employed in this work were derived from an excysted Spathidium that was isolated from a laboratory aquarium into which had been put, from time to time, the Spathidia discarded from the pedigree cultures under observation. Presumably, indeed almost certainly, the excysted animal was a descendant of those used in previous experiments.

The definitive pedigree lines were derived from one animal of a pair of conjugants among the descendants of the animal above mentioned. The four lines comprising the culture were carried on by daily isolation in standard beef extract, with Colpidia as food. For details of the method,

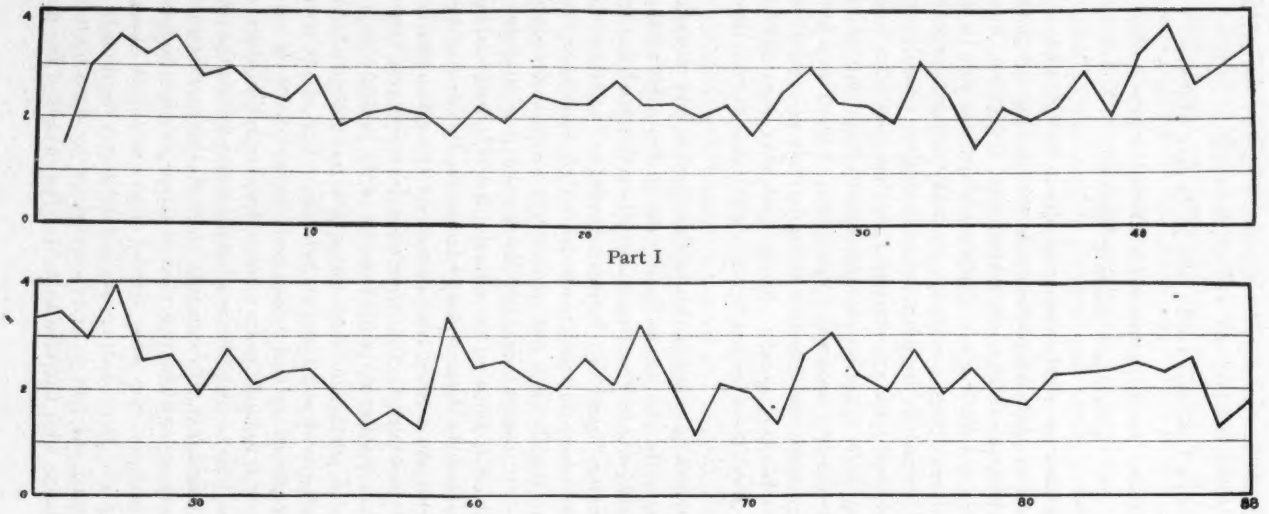

FIGURE I

Graph of the rate of reproduction of a pedigree race of *Spathidium spathula* during 1088 generations. The ordinates represent the average daily rate of division of the four lines of the culture, again averaged for five-day periods. The figures below (10, 20, etc.) indicate the numbers of the five-day periods.

reference may be made to our previous papers. During the early part of its history the culture was employed for a series of experiments,[4] and after their completion it was continued for the study of longevity without endomixis or conjugation.

The culture was carried from October 12, 1922, to December 30, 1923, when it was lost. Its life, therefore, comprised 444 days during which time 1080 generations were attained without endomixis or conjugation.

Inspection of the accompanying graph, giving the average daily division rate of the four lines of the culture throughout the 88 five-day periods of its life, shows the usual fluctuations, which are due, in part, to unavoidable environmental changes in temperature and food supply. From five-day period 36 to the end, no attempt was made to keep the culture conditions constant. Though beef extract was continued as the basic culture medium the Colpidia were taken from various sources and Colpoda were also sometimes present in the food supply. In period 53 the culture was transferred from New Haven to the Woods Hole Marine Biological Laboratory, and in period 65 it was brought back to New Haven. Following each of these transfers considerable difficulty was experienced in getting the culture established. All of these changes are registered in the rate of division. The graph gives no indication of depression periods which can be interpreted as "cycles," or of a continuous, gradual decline in reproductive activity as the culture proceeds. The slightly higher rate during the early periods is undoubtedly accounted for by the extreme precautions which were then taken with the food supply since, as previously stated, the culture was being used in other experiments.

But the culture was terminated by death in period 88, which naturally raises the question whether or no this was the result of inherent exhaustion due to long multiplication without endomixis or conjugation. All the evidence points unequivocally to a negative answer. The culture up to period 88 was continuing the even tenor of its way, when the animals suddenly appeared abnormal and all four lines died within two days without further division. Apparently it is just such an incident as occurs at intervals in all extensive pedigree work with Infusoria, and had occurred before in the present work. Previously, however, this culture had not been exterminated because the main lines were reëstablished from subsidiary lines started daily and specifically carried as "stock" for emergencies of this kind. In the present instance, owing to the pressure of other experimental work, this precaution unfortunately had been omitted, and fatal results followed.

In brief, our experience in this laboratory with pedigree cultures of Spathidium has shown progressive lengthening of life, without endomixis or conjugation, as the subtle conditions necessary for the welfare of this highly specialized species have been more nearly attained. The present

culture has demonstrated, at least, that more than one thousand generations may be attained, and makes practically positive the conclusion that *Spathidium spathula* can reproduce indefinitely without recourse to endomixis or conjugation when entirely suitable environmental conditions are supplied.

[1] Dujardin: *Histoire naturelle des Zoophytes: Infusoires*, Paris, 1841; Maupas: "Recherches expérimentales sur la multiplication des Infusoires ciliés," *Arch. zool.ex péru. gén.*, 2me sér., **6**, 1888; Calkins: "Death of the A-series of Paramecium Caudatum; Conclusions," *J. Exper. Zool.*, **1**, 1904; Woodruff: Two Thousand Generations of Paramecium, *Arch. Protistenk.*, **21**, 1911; Woodruff: "The Present Status of the Long-continued Pedigree Culture of Paramecium Aurelia at Yale University," *Proc. Nat. Acad. Sci.*, **7**, 1921.

[2] Woodruff and Spencer: "Studies on Spathidium Spathula, II, The Significance of Conjugation," *J. Exper. Zool.*, **39**, 1924. Moore: "Endomixis and Encystment in Spathidium Spathula." *Ibid.*, **39**, 1924.

[3] Calkins: "Uroleptus Mobilis. III. A Study of Vitality," *J. Exper. Zool.*, **31**, 1920. Calkins: "What did Maupas Mean?" *Amer. Nat.*, **57**, 1923.

[4] Moore: "A Further Contribution to the Study of the Effects of Conjugation and Encystment in Spathidium Spathula," *J. Exper. Zool.* (in press).

A POSSIBLE EXPLANATION OF TERTIARY LINE SPECTRA IN X-RAYS

BY DAVID L. WEBSTER

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Communicated March 10, 1924

In two recent numbers of these PROCEEDINGS, Clark and Duane[1] report the discovery of unexpected peaks in the ionization spectra of secondary X-rays. A pair of these are shown in the accompanying figure, where the curve with the experimental points is copied from Clark and Duane's second paper, and represents the spectrum of tungsten rays scattered from molybdenum. The tungsten K lines show in their usual positions, in two orders; and the molybdenum K lines, which are off the scale of angles in this figure, are shown in the original. The two unexpected peaks are the ones between the two orders of the tungsten lines.

Clark and Duane explain these peaks as follows: A K electron in a molybdenum atom absorbs a quantum of one of the tungsten lines and starts out as a photoelectron, with energy given by de Broglie's[2] law,

$$E = (h\nu' - h\nu_K)$$

where ν' is the primary frequency and ν_K is the molybdenum K-limit frequency. It then strikes other molybdenum atoms, producing a con-

tinuous spectrum, of tertiary X-rays, whose maximum frequency is

$$\nu'''_{\max} = \frac{E}{h} = \nu' - \nu_K$$

This continuous spectrum is then assumed to be so narrowed down by filtration in emerging from the molybdenum that it appears finally as a narrow peak.

This explanation is open to two serious objections.

The first, noted by Compton,[3] is that such photoelectrons could not be expected to act on other molybdenum atoms in any way different from the action of ordinary cathode rays of the same energy; and from known efficiencies of excitation of X-rays one would expect the energy of the resulting tertiary beam to be too small to observe, even without the further reduction in filtering.

The other objection is that to change an ordinary continuous spectrum to such a narrow peak would take a most excessive amount of filtering; whereas a simple calculation shows that if the primary rays enter the molybdenum at an angle θ' to the surface and the tertiary rays leave at θ''', the filtering is given by the equation

$$I''' = \text{const.} \times \frac{i''' \csc \theta'''}{\mu' \csc \theta' + \mu''' \csc \theta'''}$$

where I''' is the tertiary-ray intensity as observed through the spectrometer slits and i''' is the tertiary radiation from one atom, without filtering. Even under the most favorable circumstances, with θ''' almost zero, this amounts only to

$$I''' = \text{const.} \times \frac{i'''}{\mu'''}$$

The results of such filtering are shown in the "Thick-Target Spectra," in the lower part of the figure, where the filtered one is for a typical case, $\theta' = \theta''' = 45°$. Clark and Duane do not give their angles, but even with $\theta''' = 0$ the curve would differ from that by less than twice the width of the lettering under that curve. To explain the observed peaks in terms of such spectra is quite impossible.

We must therefore make some other hypothesis. Returning to the question of intensity, if the molybdenum atoms which the photoelectrons strike after leaving the ejecting atoms cannot give it, how about the ejecting atoms themselves? Those atoms bear a relation to the photoelectrons different from the relation of any atom to a cathode ray, and may perhaps have an unprecedented probability of radiating. In fact an analogy for this may be found in Ellis's[4] discovery of the unusual tendency for an atom emitting γ-rays to reabsorb them and give their energy to photo-

electrons; and the present assumption is almost that of a process inverse to Ellis's.

Assuming the probability of such radiation to be of a much greater order of magnitude than the very small radiation probabilities ordinarily found, one cannot calculate the resulting spectrum from the ordinary data on thick targets, where the cathode rays may make radiating impacts at any stage of their motion in the target, and therefore at any kinetic energy from their initial energy down to zero. Rather than that, one must calculate it from the spectra of thin targets, where impacts can occur only at the initial energy or not at all. For the most probable form of a thin-target spectrum, data are available in a paper by Webster and Hennings,[5] and the energies to be assumed are probably those with which the photoelectron strikes the other electrons in the ejecting atom while on its way out.

Since the L electrons are nearest the K, on the average, and the characteristic emission spectra show that they are the ones most likely to fall into any vacancy in the K shell, we may assume the energy for most of the impacts is not Clark and Duane's $h(\nu'-\nu_K)$, but $h(\nu'-\nu_{K\alpha})$. Likewise the next most important type of collisions is on the M electrons, with energy

$$h(\nu' - \nu_{K\beta}),$$

and finally we have a few at

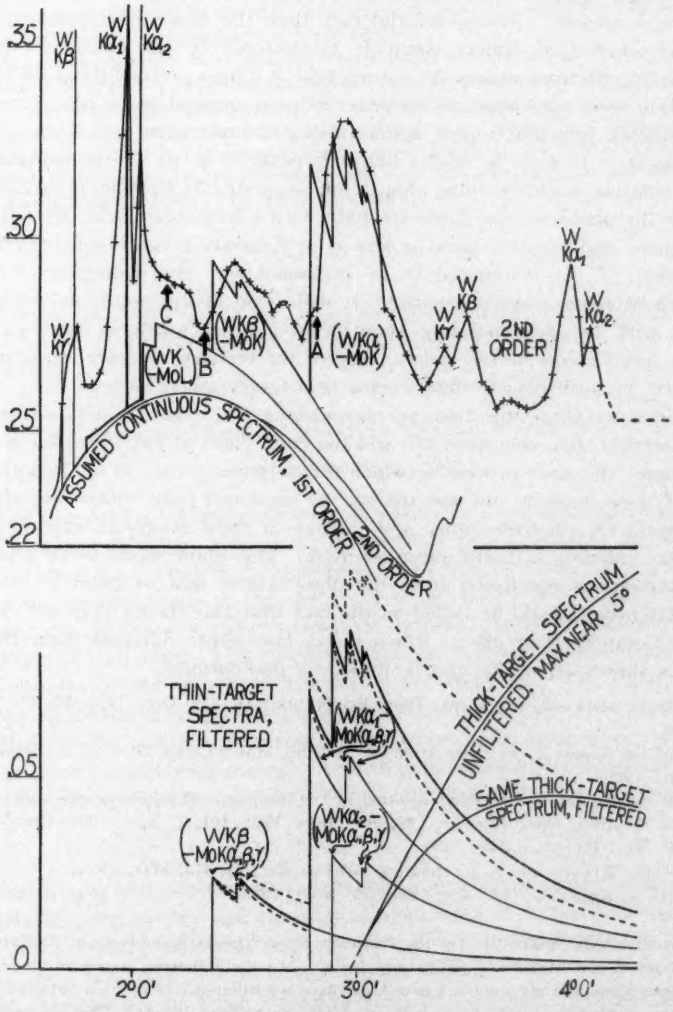
$$h(\nu' - \nu_{K\gamma}).$$

Collisions of all these types may of course occur with electrons excited by primary rays of any frequency above the molybdenum K limits, therefore especially strong spectra may be predicted for all values of ν' given by the tungsten lines. The full-line graphs, marked "Thin-Target Spectra, Filtered," in the lower half of the figure, are the predicted spectra due to all these collisions, with the same typical filtering as in the continuous spectra, and the uppermost graph of this set is the sum of the lower ones. The dotted graphs show the spectra predicted for $\theta''' = 0$ (maximum filtering) and $\theta' = 0$ (no filtering) and give an idea of the extent to which filtering can be expected to modify such spectra under different conditions.

Assuming a reasonable curve for the continuous spectrum due to other sources, and adding to it a spectrum calculated more or less as above for the effect of the molybdenum L photoelectrons, and finally adding these spectra due to the molybdenum K photoelectrons, we get the theoretical spectrum plotted with the experimental one in the upper half of the figure.

The agreement is of course not perfect, even with allowances for effects of slit widths, and it would not be made very much better by the assumption of maximum filtering. But one would hardly expect it to be perfect,

since the conditions of radiating collisions under these special circumstances are not those of the experiments with cathode rays from which Webster and Hennings' thin-target spectra were calculated.



The most serious objection to this theory is probably not this minor disagreement with the observed spectra, but the question whether a collision between two electrons can give rise to radiation carrying off all

or nearly all of their energy. Considerations of conservation of momentum, suggested by Ehrenfest in a discussion of this subject, appear adverse to this idea, unless the electron with which the photoelectron collides may have opposite momentum or be constrained in some unforseen way by the nucleus. Moreover, the fact that the classical electrodynamic theory indicated almost complete interference in the radiations from colliding electrons makes the assumption of a high probability of such radiation seem somewhat at variance with the general ideas of the correspondence principle, unless, again, unforseen constraints act on one of the electrons. It may be that a better hypothesis as to the actual process of radiation would be found along a line suggested by Epstein, of assuming that the photoelectron starts its flight with a hyperbolic orbit around the nucleus, and radiates more or less as in Kramers's[6] continuous-spectrum theory. If so, it remains to be explained why the deduction of $h\nu_{K\alpha}$ must be made from its energy. A deduction of $h\nu_K$ would be more in line with the corresponding quantity in Kramers's orbits, but $h\nu_{K\alpha}$ fits the observations better, unless, indeed, the tertiary rays are much more nearly monochromatic than even a thin-target spectrum.

However, these objections are on somewhat uncertain grounds, and more theoretical than experimental; and the chief point at issue thus far is not so much the exact process by which the radiation occurs as the hypothesis that these tertiary line spectra can be produced only within the atoms ejecting the photoelectrons, and involve in these atoms an efficiency of X-ray emission hitherto unprecedented. This point seems to be a fairly unambiguous conclusion from the observations now at hand.

Attention should be called to the fact that this theory does not cover the Compton-Ross effect, which obeys laws quite different from these, as is shown especially clearly in Ross's photographs.

[1] G. L. Clark and W. Duane, These PROCEEDINGS, **9**, 419, Dec., 1923; **10**, 41, Jan., 1924.

[2] M. de Broglie, *C. R. Paris Acad. Sci.*, Jan. 31, Mar. 29, Sept. 26, 1921; *J. Physique.*, Sept., 1921.

[3] A. H. Compton, December meeting of the American Physical Society, 1923.

[4] C. D. Ellis, *Proc. Roy. Soc.*, **99**, 261, June, 1921; **101**, 1, Apr., 1922; *Proc. Cmb. Phil. Soc.*, **21**, 121, May, 1922.

[5] D. L. Webster and A. E. Hennings, *Physic. Rev.*, **21**, 312, Mar., 1923.

[6] H. A. Kramers, *Phil. Mag.*, **46**, 836, Nov., 1923.

NOTE ADDED APRIL 21: *On the Necessity for a Special Hypothesis to Explain the Tertiary X-Ray Spectra of Clark and Duane.*—In the following paper of which the authors have sent me a copy, I note that there is a difference of opinion between us as to the short-wave limits to be estimated for the tertiary bands. This suggests the desirability of a careful, critical analysis of the general problem of estimating short-wave limits in spectra containing overlapping bands, but it is more desirable at this stage to get more complete data first, and I am glad to see that Clark and Duane propose to try further experiments on this point.

It must be remembered, however, that the exact values of the short-wave limits are not the essential point of the theory by which I have attempted to explain these tertiary bands. As noted above, it is immaterial for this theory whether its basic hypothesis is so framed as to lead to the use of an emission line frequency in the calculation, or the frequency of an absorption limit. In other words, it is immaterial, so far as the essential point is concerned, whether the short-wave limits of the bands are where I estimated them or at the positions given by Clark and Duane.

The essential point is, first, that the unexpectedly high intensity and the narrow wave-length range of these bands are both inexplicable on the simple theory that the bands are ordinary continuous spectra, produced by the impacts of photoelectrons on atoms struck after leaving the atoms from which they are ejected; and furthermore, that both these characteristics of the bands are explained at once by the hypothesis outlined in my paper. As this point has not been discussed by Clark and Duane, one may infer that there is no disagreement about it.—D. L. W.

ON THE THEORY OF THE TERTIARY RADIATION PRODUCED BY IMPACTS OF PHOTO-ELECTRONS

BY GEORGE L. CLARK[1] AND WILLIAM DUANE

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Communicated April 4, 1924

The authors have published[2] recently accounts of experiments in which the tertiary radiation due to the impacts of photo-electrons produced by primary X-rays has been discovered. The tertiary radiation appeared in some investigations of the spectrum of secondary rays coming from secondary radiators of various chemical elements under the influence of primary X-radiation. The tertiary radiation occupies certain bands in the spectra of the rays from the secondary radiators. The theory we proposed for the tertiary radiation assumed that it was due to the electrons emitted by the primary rays from certain atoms when these electrons impinged upon neighboring atoms. According to this theory, the short wave-length limit of a tertiary radiation band ought to be given by the equation

$$\nu = \nu_1 - \nu_2, \qquad (1)$$

in which ν, ν_1 and ν_2 represent the frequencies of the limit of the tertiary band, the primary ray and the critical absorption of a chemical element in the secondary radiator, respectively. The experimental data agree with the equation to within errors of measurement.

In this number of these PROCEEDINGS, D. L. Webster, in an article submitted by him to us for comment, publishes a slightly different theory for the origin of the tertiary bands. According to his theory, the tertiary radiation arises from the impact of a photo-electron against a second elec-

tron in the very same atom from which it came. Assuming with Webster that the entire amount of energy of the photo-electron at the time it hits the second electron may appear as energy of radiation, the short wave-length limit of a tertiary band should be given by the equation

$$\nu' = \nu_1 - \nu_3 \qquad (2)$$

where ν_3 represents the frequency of vibration of an emission line in the X-ray spectrum of the chemical element, instead of a critical absorption frequency.

On account of the importance of this interesting idea the experimental data at hand should be examined with great care to see whether they fit one theory better than the other.

The difference between the high frequency limits of the tertiary band calculated on the two theories is the difference between a critical absorption frequency and that of an emission line. It is desirable, therefore, to choose as test data the results of the experiments in which this difference has its greatest value. The difference between the critical absorption frequency and that of a given emission line in its series increases rapidly with the atomic number of the chemical element. Silver has the highest atomic number of those chemical elements for which we have as yet observed the tertiary radiation. Let us, therefore, examine the theory as applied to the tertiary radiation from silver, and let us take the case in which the photo-electron is expelled from the K level of a silver atom by an X-ray belonging to the $K\alpha$ line of tungsten. Transforming equations 1 and 2 by introducing wave-lengths we find

$$\lambda = \frac{\lambda_1\lambda_2}{\lambda_2 - \lambda_1} \qquad (3) \qquad \text{and} \qquad \lambda' = \frac{\lambda_1\lambda_3}{\lambda_3 - \lambda_1} \qquad (4)$$

as the expressions for the short wave-length limits of the tertiary radiation band according to the two theories. In these expressions λ_2 signifies the K critical absorption wave-length of silver and λ_3, the wave-length of the $K\alpha$ doublet of silver. Putting their known values into the equation and that of the $K\alpha_1$ line of tungsten, we find for the short wave-length limit $\lambda = .366$ Å according to our theory and $\lambda' = .330$ Å according to Webster's. The short wave-length limit of the tertiary band as determined by experiment is contained in table II of our note to which Webster refers. It is $\lambda = .365$. It appears, therefore, that the experimental wave-length agrees very closely with its value calculated according to our theory and does not agree at all with that predicted by Webster's theory. In fact, the wave-length calculated according to Webster's theory lies about half way between the short wave-length limits of the tertiary bands due to the radiation from the $K\alpha$ line of tungsten and the $K\beta$ line of tungsten. As these differ from each other by as much as .07 Å, it is impossible that

we could have made an error as great as that of the difference between Webster's theoretical value and the experimentally observed value. These wave-lengths can be measured to within about .002 Å.

A similar calculation of the short wave-length limit of the tertiary radiation due to the $K\beta$ line of tungsten gives $\lambda = .297$ according to our theory and $\lambda = .274$ according to Webster's. The value determined by our experiments is $\lambda = .296$, which agrees with our theoretical value to within $^1/_2\%$ but differs from Webster's by 8%, which is an impossible error.

An examination of the curve representing our experimental data for silver, published in the March number of these PROCEEDINGS, on page 92, shows clearly that the tertiary humps are very sharply marked on their short wave-length sides and that they are separated from each other by a considerable interval.

If we calculate the angles corresponding to the short wave-length limits, we find that the humps representing tertiary radiation due to the tungsten $K\alpha$ line should begin to rise at the angle 197°45′ according to our theory and 198°5′ according to Webster's. The position 197°45′ appears to be exactly correct, whereas the value 198°5′ lies half way between the short wave-length limits of the two tertiary radiation humps. The line between the two humps is substantially a smooth curve and shows no indication of any excess radiation at a point half way between the two limits. Similarly, the short wave-length limit of the hump representing tertiary radiation due to the tungsten $K\beta$ line ought to lie at the angle 198°24$^1/_2$′ according to our theory and 198°38′ according to Webster's. It appears from the curve that the hump begins at 198°26′ which agrees with our theoretical value and there is no excess radiation indicated at 198°38′. In making these calculations, a slit correction has been added amounting to about 5′. Even though this slit correction were entirely neglected, the positions of the two limits of the tertiary radiation, according to Webster's theory would be shifted only a very short distance on the curve and would not approach the experimentally determined positions to within anything like experimental errors. Hence, there is not the slightest evidence for any radiation that could be attributed to the impact of photo-electrons against other electrons in the atoms from which they come, as assumed by Webster.

Passing now to the data for the experiments with molybdenum reported in our note in these PROCEEDINGS for January, 1924, we find that Webster's theory predicts that the hump representing tertiary radiation from the tungsten $K\beta$ line should begin at 203°10′ whereas our theory predicts that it should begin at 203°25′. The actual beginning as nearly as can be estimated on the curve lies at 203°24′ and agrees therefore, much more closely with our theory than with Webster's. Similarly, the observed

position for the short wave-length limit for the hump representing tertiary radiation from the tungsten $K\alpha$ line, lies at 203°54′. The value calculated by our theory is 203°55′ and by Webster's 203°44½′. Evidently in this case also, our theory represents experimental values far more closely than Webster's.

The difference between the calculated values of the short wave-length limits of the tertiary radiation according to the two theories grows smaller and smaller as we proceed to chemical elements of lower atomic number, for the difference between the critical absorption frequency and that of an emission line in its series decreases as the atomic number decreases. Even for the germanium experiment, described in the March number of these PROCEEDINGS, the two theories agree with each other quite closely. When we reach the chemical elements from carbon to chlorine, the difference between the two theories becomes inappreciable. Both theories predict tertiary radiation substantially as we have found it in our experiments described in the April number of these PROCEEDINGS. In these experiments, we used a molybdenum target tube and secondary radiators containing lithium, carbon, oxygen, sodium, aluminium, sulphur and chlorine. The experiments with carbon were very similar to the well-known experiments by A. H. Compton. In every case we found evidence of tertiary radiation substantially in the position predicted by our theory and, therefore, also substantially in the position predicted by Webster's modification of it.

The extremely interesting and painstaking experiments performed by Ross with molybdenum primary rays and a carbon secondary radiator supply photographic evidence of radiation having wave-lengths longer than those of the characteristic lines of molybdenum. Evidently the photographic plate registers the *tops* only of the peaks obtained by ionization experiments. It would require a very much longer time of exposure to obtain the position of the short wave-length limits of the tertiary radiation. To make sure that a limit was recorded the exposure should be long enough to record the continuous spectrum. In particular the photograph indicates the shift in the position of the short wave-length limit of tertiary radiation as the angle between the primary and secondary rays decreases. Such a shift for tertiary radiation has been observed by us in our experiments with both tungsten rays and molybdenum rays.

In the December, 1923, number of these PROCEEDINGS, we published accounts of experiments with the K series lines from a tungsten target as the primary radiation and with a secondary radiator of carbon. In these experiments scattered radiation from the carbon appeared with wave-lengths precisely equal to those of the primary K series lines of tungsten, but no evidence of radiation comparable in intensity with the scattered rays and having wave-lengths somewhat longer than the tungsten lines was

detected. The last number of the Comptes Rendus to reach us contains a note by M. de Broglie, in which he describes similar experiments with a tungsten target tube but with a secondary radiator consisting of a combination of carbon and tungsten. He employed the photographic method of registering the secondary radiation, and on the photographic plate appeared a line separated from that representing the fluorescent radiation of tungsten, on its long wave-length side. The two experiments together suggest that this radiation with shifted wave-length comes from the tungsten and not from the carbon. As a matter of fact, our theory of tertiary radiation predicts that, in this case, there should be radiation of somewhat longer wave-length than the primary radiation, due to the impact of electrons ejected by the primary K series lines of tungsten from the L levels of the tungsten atoms in the secondary radiator. If we substitute in formula 3, which represents our theory, the known values for the wave-length of the $K\alpha$ doublet of tungsten and for the $L1$ and $L2$ critical absorption wave-lengths of tungsten, we find that the short wave-length limit of the tertiary radiation should lie at the wave-lengths .2521 and .2601, respectively. There should, therefore, be radiation coming from the carbon-tungsten secondary radiator having maxima of intensity at wave-lengths slightly longer than these values. This radiation lies precisely in the region of the spectrum occupied by the line with shifted wave-length mentioned in de Broglie's note.

Although the experiments carried on so far with chemical elements of low atomic number cannot furnish decisive evidence between our theory and Webster's modification of it, the experiments with chemical elements of higher atomic number show clearly that there is no perceptible radiation which has the wave-lengths predicted by this modification. On the other hand definite evidence appears in our experiments of radiation having the short wave-length limits predicted by our theory that it arises from the impacts of photo-electrons against neighboring atoms.

It would, of course, be possible to devise a theory somewhat different from Webster's and consistent with equation I, which represents experimental facts. We might assume, for instance, that either the electron directly acted upon by the primary radiation or the electron in its own atom that it impinges against must get completely outside of the atom in order that tertiary radiation shall take place. In this case the minimum amount of energy absorbed by the atom would be $h\nu_2$, and, equating this to the differences between the $h\nu$ values of the primary and tertiary radiation, we get

$$h\nu_2 = h\nu_1 - h\nu,$$

which reduces immediately to equation I. This hypothesis, however, seems somewhat artificial, unless in the future it becomes advisable to

assume in general that an atom cannot radiate even continuous spectrum radiation without changing or being changed from one stable state to another.

[1] NATIONAL RESEARCH FELLOW.
[2] These PROCEEDINGS, Jan., March and April, 1924.

ABSORPTION MEASUREMENTS OF CERTAIN CHANGES IN THE AVERAGE WAVE-LENGTH OF TERTIARY X-RAYS

BY SAMUEL K. ALLISON[1] AND WILLIAM DUANE

Communicated March 28, 1924

In a series of experiments by G. L. Clark and William Duane[2] on the wave-lengths of secondary X-rays, the radiation from the secondary radiators has been found to contain tertiary radiation with a high frequency limit ν given by the equation

$$\nu = \nu_1 - \nu_2$$

where ν_1 is the frequency of the primary radiation and ν_2 a critical absorption frequency of the secondary radiator. The experiments on the spectrum of secondary X-rays at different angles from the primary beam showed that, while the short wave-length limit of the tertiary radiation remained fixed, the maximum of the "hump" shifted toward longer wave-lengths with increasing angle. This effect is known to occur in the primary general radiation when examined at various angles from the incident cathode rays.[3] The radiation resulting when ν_1 is the frequency of the $K\alpha$ doublet of the target substance and ν_2 the K critical absorption frequency of the secondary radiator has a greater intensity than any other and our experiments are limited to this case.

We have examined the shift of the maximum of the tertiary radiation by the ingenious method described by Ross.[4] A beam of X-rays entering through the usual slits fell upon the secondary radiator, placed in the center of the spectrometer table. A screen was selected with a K critical absorption wave-length slightly longer than the short wave-length limit of the tertiary radiation under consideration. This screen was placed between the ionization chamber and the secondary radiator, and the ionization current examined at various angles with the direct beam. The shift of the maximum of the tertiary is indicated by an increase, rapid at first and then slow, of the fraction of the radiation transmitted by the screen as the angle increases. The wave-length width of the tertiary radiation is fairly narrow, but still broad enough; so that the effect should not be

sharp and the angle at which it occurs cannot readily be calculated from the wave-lengths used.

In these experiments we employed a tungsten target tube, operated at 90,000 volts and 2.2 milliamperes. The secondary radiator consisted of a silver plate, one surface of which had been highly polished. This plate had been used by G. L. Clark and William Duane in their experiments on the spectrum of tungsten rays scattered from silver. The secondary radiation from this plate contains tertiary radiation with a short wave-length limit of .3660 Å, due to the impact of electrons ejected from the K level of the silver atoms by X-rays having the wave-length of the α-doublet of tungsten.

The following table indicates the screens selected for the study of the silver tertiary radiation.

SCREENS SELECTED FOR AG TERTIARY (SHORT WAVE-LENGTH LIMIT .366Å)

SCREEN	K CRITICAL ABS.	$\Delta\lambda_i$	$\Delta\lambda$
Iodine	.3737 Å	.0077 Å	.164 Å
Tellurium	.3896	.0236	.180
Antimony	.4065	.0405	.196

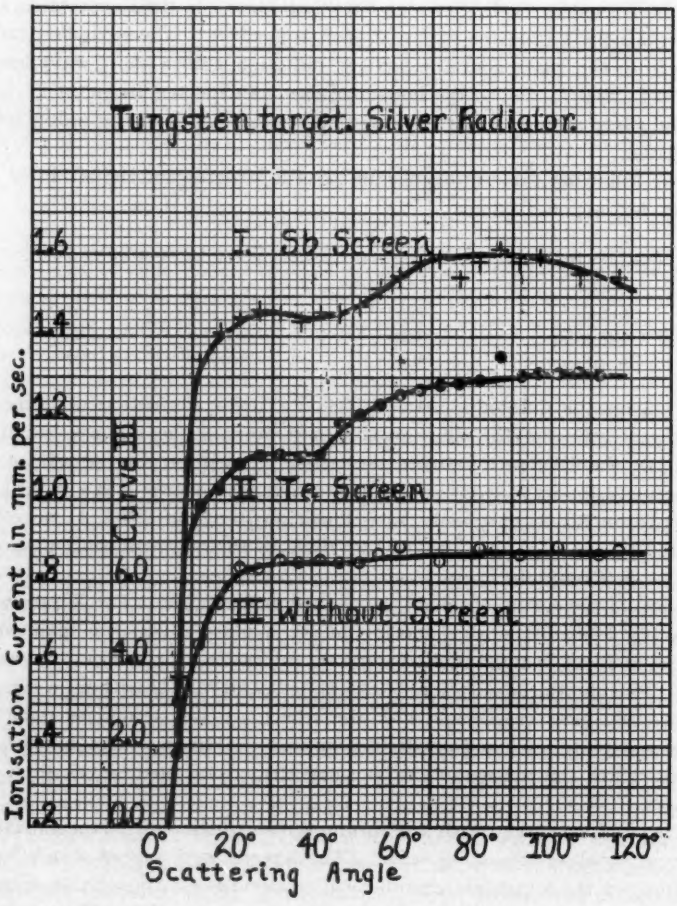
$\Delta\lambda_i$ means the difference between the K critical absorption wave-length of the screen, and the short wave-length limit of the tertiary radiation and $\Delta\lambda$, the difference between the critical absorption wave-length of the screen and the wave-length of the primary tungsten rays ($WK\alpha$ = .210 Å).

Since for all these screens $\Delta\lambda$ is much greater than .0484 Å, no effect should be expected that could be ascribed to the quantum shift proposed by A. H. Compton.[5]

Some of the curves obtained are plotted in the figure. The X-rays were reflected from the polished face of the silver plate. This was inclined 5° to the incident beam, and all the curves fall off to nearly zero at an angle of 5° due to the absorption of the plate. Curve III (without a screen) shows that between 20° and 120° the intensity of the secondary radiation from this plate remained practically constant. This makes it unnecesary to calculate the absorption ratio since the curves themselves show the breaks.

Curve II was taken with a tellurium screen which reduced the ionization current to about $^1/_6$ of its value. The screens were placed about 2 cm. in front of the ionization chamber slit to prevent an appreciable amount of their secondary radiation entering the chamber. Curve II shows that at an angle of about 40° an appreciable amount of energy in the tertiary radiation shifts over into wave-lengths greater than .3896 Å and is, therefore, less absorbed by the tellurium. Curve I shows the effect with an antimony screen. An iodine screen did not produce the effect, presumably because in this range of angles too much energy already existed in

the tertiary wave-lengths longer than the iodine critical absorption wave-length. The angles at which the effects appeared and their magnitude depended greatly upon the angle at which the primary beam struck the silver plate. It appears that in scattering from elements as heavy as



silver, the shape of the tertiary hump depends to a considerable extent upon the filtration.

Tungsten rays were also scattered from graphite and the experiment carried out with a dysprosium screen ($\lambda_{abs.}$ = .2308 Å). No effect could be detected at scattering angles between 20° and 120°. A peak shifting

according to the Compton equation should produce this effect between 80° and 90°. Apparently little change in the energy of the tertiary radiation from carbon (short wave-length limit .211 Å) due to the tungsten $K\alpha$-doublet takes place in wave-lengths as long as .2308 Å in this scattering range.

From the results of the experiments with a silver radiator and tungsten primary rays, we would expect the observed effects to occur in any experiment in which the scattered radiation was examined at various angles, with a screen whose critical absorption wave-length had the proper value, and provided that the wave-lengths of the tertiary radiation differed sufficiently from that of the primary.

[1] NATIONAL RESEARCH FELLOW.
[2] *Proc. Nat. Acad. Sci.*, **9**, 413, 419; **10**, 41; et seq.
[3] Wagner, *Phys. Z.*, **21**, 621 (1920); **23**, 503 (1922).
[4] Ross, Proc. Am. Phys. Soc., *Physic. Rev.*, **22**, 525 (1923).
[5] A. H. Compton, *Physic. Rev.*, **21**, 483 (1923).

THE SERIES SPECTRA OF THE STRIPPED BORON ATOM (BIII)

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In a preceding article[1] we have brought forward evidence that in "hot spark" spectra the strongest lines generally correspond to atoms from which the valence electrons have all been stripped off, so that the resulting spectrum is hydrogen-like, i.e., is due to one single electron moving between the series of levels characteristic of a simple nucleus-electron system.

For such a nucleus-electron system the Bohr theory in its elementary form[2] which dealt only with circular orbits, i.e., with variations in azimuthal quantum numbers, the radial being always zero, yielded at once the result that the energies corresponding to a given quantum state, e.g., quantum number 1, increased in the ratio 1,4, 9, 16, etc., as the nuclear charged increased in the ratio 1, 2, 3, 4, etc. This meant physically that the frequencies corresponding to jumps from infinity to an orbit of given quantum number, technically called term-values, when divided by the square of the nuclear charge should come out a constant; otherwise stated that the constant term in the Rydberg formula should become N, 4N, 9N, 16N.

After the introduction by Sommerfeld of radial in addition to Bohr's azimuthal quantum numbers the foregoing conclusions were obviously

still applicable to the series of *circular* orbits corresponding to a given azimuthal quantum number and the varying nuclear charges 1, 2, 3, 4, etc.

These conclusions have recently been shown by Bohr,[3] Paschen,[4] and Fowler[5] to check with considerable accuracy with the experimental energy levels obtained through the study of the optical spectra produced by the stripped atom of sodium (Na_I), Magnesium (Mg_{II}) and Aluminum (Al_{III}) as the following f term-values (energy levels) when divided through by 9 for Al, 4 for Mg, and 1 for sodium show. The four 4_4 circular orbits (the large 4 meaning total and the subscript azimuthal quantum numbers) obtained from elementary Bohr theory and the observed fundamental series in Na_I, Mg_{II}, Al_{III} give for the foregoing ratios.

Theory 6858.44, Na_I 6860.4, Mg_{II} 6866.8, Al_{III} 6871.28.

The agreement between the elementary Bohr theory and experiment for the 6_6, 5_5 orbits with all these metals is even better, and that for the 3_3 almost as good.

Now the most conspicuous of Bohr's very recent contributions to the physics of the atom has consisted in the systematic analysis of the departures from the foregoing elementary theory which are to be expected from the *assumption* of the penetration (especially because of orbits of high ellipticity) of the radiating electron into the inner regions of the atom where it is no longer shielded from the nucleus by the inner shell, and where, therefore, the effective value of the nuclear charge is suddenly increased and with it the numerical value of the total energy of the orbit,[6] and hence the numerical value of the frequency corresponding to that orbit. These term values, divided by n^2, for the highly elliptical orbit 3_1, as shown herewith, bring out sharply the departures in the case of this orbit from the value given by the elementary theory and the progression of this departure.

Elementary theory 12192.78, Al_{III} 25494.89, Mg_{II} 30316.9, Na_I 41449.00.

It will be seen that all these term-values (energy levels) are systematically larger than that given by the elementary theory.

By applying the foregoing considerations to the computation of the spectrum to be expected from the stripped atom of boron, B_{III}, it should be possible to predict with the greatest accuracy the spectral lines due to jumps between the circular orbits of the larger radii. Thus, the jump between the circular orbits 5_5 and 4_4 ($4f - 5f'$) should give a line of frequency

$$\nu = 9N_B(1/4^2 - 1/5^2) = 9 \times 109732.(1/16 - 1/25) = 22221 \text{ or } \lambda = 4500.3 \quad (1)$$

In the early stages of this work the boron spectrum had been obtained for short wave-lengths inside the vacuum spectrometer and simultaneously

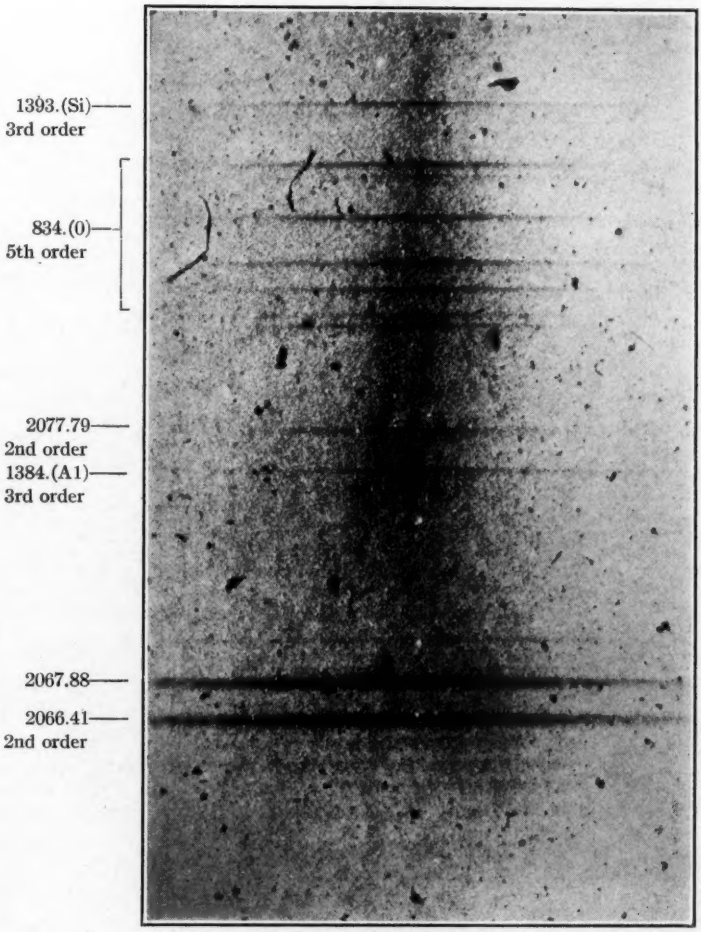
1393.(Si)
3rd order
834.(0)
5th order
2077.79
2nd order
1384.(A1)
3rd order
2067.88
2066.41
2nd order

FIGURE 1

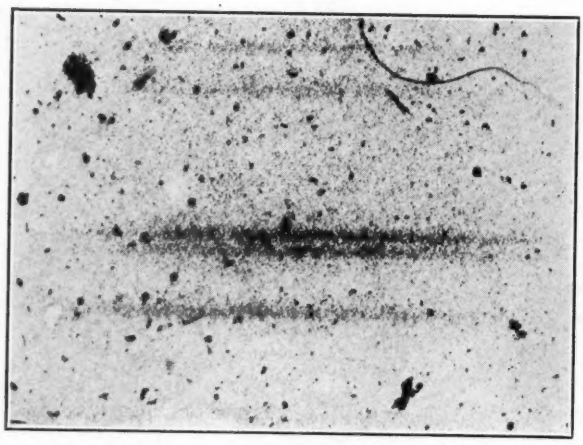

FIGURE 2

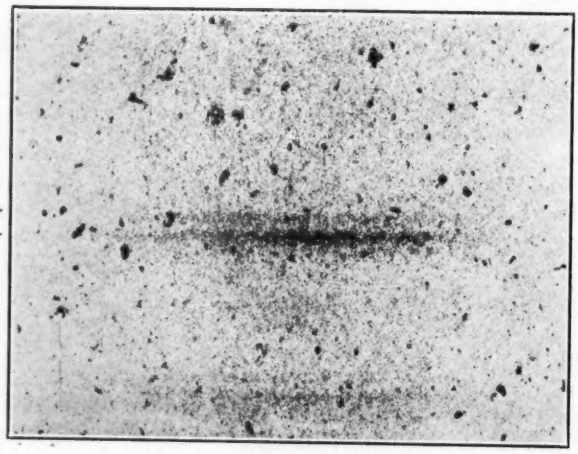

FIGURE 3

for long wave-lengths outside, the light coming through a quartz spectrograph. A study of the plates taken with the aid of this quartz spectrograph revealed at once a hitherto unidentified line at $\lambda = 4499.0$. *To an accuracy of one part in three thousand this is in exactly the spot predicted by the elementary theory.*

Again, the jump between the circular orbits 4_4 and 3_3 ($3d - 4f$) should be given by the foregoing formula when the 5 and 4 are replaced by 4 and 3, respectively. The carrying through of this computation yields $\lambda = 2083$. But since the departure shown by Paschen and Fowler from the elementary theory becomes greater the smaller the orbits (presumably because the electrons of the k shell begin to exert influences appropriate to their actual position outside of instead of in the nucleus) it should be more correct to attempt to predict the position of the boron line here being sought by simply dividing through by $3^2 = 9$ the wave-length of the corresponding lithium line at $\lambda = 18697$ A. This gives the predicted position at 2077.4 A.

Plate No. 1 was taken for the sake of studying with high resolution hot-spark vacuum spectra from electrodes containing boron the wave-length region about $\lambda = 4150$ A. which region should contain the second order of the predicted line at 2077, the existence of which had never been brought to light by any preceding observers. *This plate reveals the line sought and gives it a wave-length of 2077.79, within about one part in 5000 of the predicted position.*

The next jump to be expected in B_{III} is that from the 3_3 to the 2_2 orbit ($2p - 3d$). But in the alkali type of spectra there are always two p terms, p_1 and p_2 giving rise to a doublet $2p_1 - 3d$ and $2p_2 - 3d$, so that the line to be here expected is a close doublet. Its very approximate wave-lengths should be given by substituting 2 and 3 for the 4 and 5 of formula (1). This gives 729 A. But again the prediction should be more accurate by dividing the corresponding lithium doublet, which is at 6103 A, by 9. This gives 678 A, which is within about an angstrom of the strongest boron line below 1300 A. which we have already published in our "Extreme Ultra-violet Spectra,"[7] in which work there had been no indication that the line was a doublet. *In new studies on the structure of this line made with the use of a very narrow slit (.02 mm.) and with spectra of order from the third to the sixth the line was definitely revealed as a close doublet in all of the plates and orders studied.* Fig. 2 of the plate shows a photograph of this doublet in the sixth order. The average separation of the doublet as determined from measurements on several different plates and in four different orders is .15 A. correct to at least .01 A. The resolution here shown has so far as we know never been obtained in any preceding work in the extreme ultra-violet. The corresponding frequency separation is $\Delta\nu = 32.7$ cm.$^{-1}$.

There should of course also be a doublet of this separation caused by a jump to this same pair of levels p_1 and p_2 from the 3_1 orbit. This should be the first term of the sharp series of B_{III}, i.e. $(2p_1 - 3s)$ and $(2p_2 - 3s)$. A study with our high resolution of the fine structure of all the strong lines of boron below 2000 A.—there are but six of them—revealed but one other doublet, namely 758.47 − 758.68, which had the same frequency separation. *This definitely identifies, then the line, the wave-length of which we had previously published at 758.5 as the first term of the sharp series of* (B_{III}).

There should be one further jump giving rise to another doublet of this same frequency difference, namely, the jump from the p_1, p_2 pair of levels to the $2s$ orbit, and since this would be the first term of the principal series it should be the strongest existing (B_{III}) line. We had already located this strongest boron line at 2066.2 and 2064.2 and attributed it to the stripped boron atom[8] but had taken these wave-lengths and their separation from a preceding observer who had worked in air.

Making now a study under high resolution, in both first and second orders (see Fig. 1 of the plate for a very excellent reproduction of this doublet in the second order) we fixed these wave-lengths at 2066.41 and 2067.88, which corresponds to a frequency separation of 34.4. *This exhausts all the* B_{III} *lines which can be expected to appear in any strength, and all of them have been found where they should be and with the right separation for all the predicted doublets.*

The separation of the 677 doublet (32.7) is a trifle less than the much more accurately determined separation (34.4) of the $2s - 2p$ line. The difference is only 5% and may be accidental, since the estimated accuracy of .01 A. amounts in .15 A. to 7%, but it is interesting to note that a corresponding discrepancy was found for the same line in lithium and in this case given an interesting explanation by Fowler.[9]

Table I gives the complete results of our study with high resolution of the fine structure of all of the strong boron lines which we have brought to light, only two of these lines, the pairs at 2066 and at 2090, having been previously mentioned by other observers. It will be seen that the lines at 1826 and 2090 are both doublets, but with a frequency separation which definitely identifies them with B_I, since it is the same as that given by Fowler,[10] namely $\Delta\nu = 15.3$, for the B_I lines at 2497. The 1082 and the 1624 lines are revealed by our plates as complex lines, though the resolution was not sufficient to determine their true character. Hence only the mean wave-length is given in our table.

The term values given in the last column were determined from our measurements of the wave-lengths of these boron lines, starting with the $5f'$ level, which was computed from the elementary theory and then corrected by a study of the deviations from the elementary theory of the corresponding levels in Al_{III} and Li_I.

BORON

INT.	λI. A. VAC.	ν		Δν	B_{III}	TERM VALUES
3	677.01	147708.3	$2p_2 - 3d$	32.7	2s	305938±5
3	677.16	147675.6	$2p_1 - 3d$		3s	125736±5
2	758.47	131844.4	$2p_2 - 3s$	36.5	$2p_1$	257545±5
3	758.68	131807.9	$2p_1 - 3s$		$2p_2$	257579±5
2	1081.99	92423.2			3d	109870±5
6	1362.45	73397.2			4f	61742±5
7	1624.09	61579.1			5f′	39515±5
1	1825.87	54768.4	BI	16.2		
2	1826.41	54752.2	BI			
4	1842.78	54265.8				
10	2066.41	48393.1	$2s - 2p_1$	34.4		
10	2067.88	48358.7	$2s - 2p_2$			
3	2077.79	48128.1	$3d - 4f$			
3	2089.60	47856.0	BI	15.8		
3	2090.29	47840.2	BI			
2	4499.0	22227.2	$4f - 5f'$			

[1] Millikan and Bowen, *Physic. Rev.*, **23**, 1–31, 1924.

[2] *Phil. Mag.*, **26**, p. 1, 1913.

[3] Bohr, *Ann. Physik.*, **71**, 228, 1923.

[4] Paschen, *Ibid.*, **71**, 185, 1923.

[5] Fowler, *Proc. Roy. Soc.*, **103**, 427, 1923.

[6] Since Bohr takes the energy as zero at infinity a higher numerical value of the energy—itself negative—means of course a lower actual energy in the inverse sense of the atomic number.

[7] Millikan and Bowen, *Physic. Rev.*, **23**, 7, 1924.

[8] See page 32, *l. c.*

[9] Fowler, *Report on Series in Line-Spectra*, p. 98.

[10] *Ibid.*, p. 155.